

Long-Term Trends in Benthic Macrofauna Biomass in the Upper San Francisco Estuary

Dean F Messer

Public Comments

No public comments were received for this proposal.

Collaboration Panel Review

Proposal Title

#0205: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Francisco Estuary

Final Panel Rating
above average

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

above average

Yes, proposal makes clear that subprojects are part of a larger collaborative proposal.

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

above average

Yes, all questions are addressed well.

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are

#0205: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Collaboration Panel Review

there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

above average

Yes, project manager is identified and resources and time have been set aside for management and collaboration. Missing is a process for decision making and process for overcoming barriers.

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

above average

PI has experience and management history. Team members have complementary skills.

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

adequate

Dissemination of results will include: oral and poster presentations at CALFED Science Conference and State of the Estuary, plus additional national conferences (AFS, ERF and ASLO), progress reports to CALFED, a final report, and "several peer-reviewed journal articles".

Additional Comments:

Collaboration Panel (Discussion) Review

Primary reviewer felt outreach and communication plans were above average. Secondary reviewer agrees, but gave a little

Collaboration Panel Review

higher final rating. Both came to consensus and rated it Above Average.

Technical Synthesis Panel Review

Proposal Title

#0205: Long-Term Trends in Benthic Macrofauna Biomass in the Upper San Francisco Estuary

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The investigators propose to determine wet weight of archived samples, correct for loss of tissue weight during storage through the use of published values, and convert to dry tissue weight using other published conversions. Production will be estimated from published P/B values. Using archived samples to better understand the dynamics of the ecosystem is a most worthwhile goal. However, because of some of the things said in the proposal, I am concerned about whether this goal will be achieved. Positive aspects of the proposal include: all the identification has been done by a single individual; changes in samples resulting from preservation will be accounted for; the plan for incorporating the data into BDAT and development of the Benthic Bioguide is excellent. Negative aspects include: (1) It is unclear how the method for determining biomass will be validated. At one point the authors say they will compare their approach to a single conversion factor (which is a poor method); at another point they say they will compare it with length-mass relationships developed for a couple bivalve species. It is critical that their method be validated for more species! The entire proposal rests on the validity of these data and the authors have not convinced me that they are evaluating this adequately. If a method can be validated, the project will provide extremely valuable data.

#0205: Long-Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Technical Synthesis Panel Review

(The authors should consider doing a pilot project to explore different methods and determine their accuracy. These data will be essential in any future proposal submissions.) (2) Production cannot be used as an estimate of respiration without making a lot of assumptions that are of questionable validity, although they cite a study where this has been done. (3) Ash-free dry mass is very different than dry mass; yet the authors seem to equate the two terms. This is VERY troubling and makes this reviewer worried about the depth of their understanding of this task. Although the tables provided show that they do have a good grasp of the literature and potential sources for conversion factors. (4) It is not clear what analytical approaches will be used for the trend analysis. (5) The authors appear to have been selective about the sites chosen for analysis (which is good); however, the basis for this selection was not explained. Why were these sites chosen? What functional groups will be used (suspension feeders vs. deposit feeders are all that were mentioned)? (6) Hypotheses are added as an afterthought rather than as the driver for this study. This makes me concerned about their ability to use the data being generated to test hypotheses. (7) The publication record of many members of this team is not strong. I am concerned that the product of this research will be only data in BDAT. Peer-reviewed publications using these data are essential. Overall, the goal of the project is excellent, and if my concerns could be assuaged I would increase my rating to above average; however, my concerns about the team's ability to do this in light of my concerns with the proposed methods (detailed above) makes me reduce my overall rating to adequate.

Additional Comments:

Reviewers rated it as very good, very good, and good. The crux of this proposal is the conversion from numbers to biomass. Reviewers expressed significant concerns about the way this task is being approached by the team. Reviewers noted that biomass measures should have been incorporated into the monitoring plan long ago, so these data are long overdue. One reviewer noted "Biomass information is fundamental to any full appreciation of benthic processes." No discussion of methods

Technical Synthesis Panel Review

to be used in trend analysis (and relevant statistical considerations) is a significant omission that made it difficult for reviewers to determine the value of this component. One reviewer noted: "The data analysis section has no information on the statistical methods to be used to evaluate and compare the data. This is a significant omission. As such I cannot evaluate whether the methods are sound and will produce useful results." Many concerns were raised about the numerous assumptions required to make some of the calculations proposed in this project. One reviewer did a simple lab analysis to check some of the proposed methods and concluded: " I do urge very serious appraisal of the proposed accuracy of 0.0001 g. ... This proposal would be stronger were it to offer evidence that this level of accuracy can be consistently and meaningfully achieved." Another recommended that "the authors consider conducting a few experiments to determine tissue weight loss during storage of preserved animals--they would then have much more confidence in making a correction. Second, I think the biggest problem lays in the conversion of wet weight to dry tissue weight, due to the importance of mineralized hard parts. This is especially important for bivalves--it is known that shell weight can vary independently from wet (or dry) tissue weight, and the relationship between shell weight and tissue weight changes seasonally and with body size. I strongly suggest that the authors do some destructive sampling of archived bivalves, separating shell from soft tissue. It is likely that wet tissue weight will convert nicely to dry weight and on to carbon or biomass. All bets are off if the shell is included. In my opinion, this is the weak link of the proposal, but it is easily remedied." "The project is certainly feasible, as long as authors add direct measurement of tissue wet weight to their measurements. Without this, bivalve data will be problematic." "I suspect that P/B calculations will be little better than good guesses. Too many variables contribute." One reviewer noted that "This is a potential goldmine of products." Yet all expressed substantive concerns with technical aspects of the proposed research.

The investigators propose to determine wet weight of archived

Technical Synthesis Panel Review

samples, correct for loss of tissue weight during storage through the use of published values, and convert to dry tissue weight using other published conversions. Production will be estimated from published P/B values. Using archived samples to better understand the dynamics of the ecosystem is a most worthwhile goal. However, because of some of the things said in the proposal, I am concerned about whether this goal will be achieved. Positive aspects of the proposal include: all the identification has been done by a single individual; changes in samples resulting from preservation will be accounted for; the plan for incorporating the data into BDAT and development of the Benthic Bioguide is excellent. Negative aspects include: (1) It is unclear how the method for determining biomass will be validated. At one point the authors say they will compare their approach to a single conversion factor (which is a poor method); at another point they say they will compare it with length-mass relationships developed for a couple bivalve species. It is critical that their method be validated for more species! The entire proposal rests on the validity of these data and the authors have not convinced me that they are evaluating this adequately. If a method can be validated, the project will provide extremely valuable data. (The authors should consider doing a pilot project to explore different methods and determine their accuracy. These data will be essential in any future proposal submissions.) (2) Production cannot be used as an estimate of respiration without making a lot of assumptions that are of questionable validity, although they cite a study where this has been done. (3) Ash-free dry mass is very different than dry mass; yet the authors seem to equate the two terms. This is VERY troubling and makes this reviewer worried about the depth of their understanding of this task. Although the tables provided show that they do have a good grasp of the literature and potential sources for conversion factors. (4) It is not clear what analytical approaches will be used for the trend analysis. (5) The authors appear to have been selective about the sites chosen for analysis (which is good); however, the basis for this selection was not explained. Why were these sites chosen? What functional groups will be used (suspension feeders vs. deposit feeders are all that were mentioned)? (6) Hypotheses are added as an afterthought rather than as the driver for

Technical Synthesis Panel Review

this study. This makes me concerned about their ability to use the data being generated to test hypotheses. (7) The publication record of many members of this team is not strong. I am concerned that the product of this research will be only data in BDAT. Peer-reviewed publications using these data are essential. Overall, the goal of the project is excellent, and if my concerns could be assuaged I would increase my rating to above average; however, my concerns about the team's ability to do this in light of my concerns with the proposed methods (detailed above) makes me reduce my overall rating to adequate.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Long-term trends in benthic macrofauna biomass in the upper San Francisco estuary

The proposal ranked as adequate both by primary and secondary reviewers. The work would provide potentially valuable important information. However, the proposal was unclear and the methods were inadequately validated. There were technical difficulties including confusing ash-free dry mass with dry mass. While the method proposed would be extremely valuable if it could be validated, the proposal does not clearly describe how this would be done; particularly validating the wet-to-dry conversion. The hypotheses proposed for the study were added as an afterthought and did not provide clear direction for the research. A pilot study should precede the proposed study to validate the methodology. Energy and length measurements of past samples should be included, to provide a basis for the many analyses that require energy as a currency, and to provide insight into reasons for year-to-year variations in biomass.

Final Ranking: Adequate

Technical Review #1

proposal title: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Francisco Estuary

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The goals are clearly stated and are internally consistent. In a sense, the idea is timely because of the realization that the dearth of usable data on the benthos inhibits testing of more comprehensive hypotheses on the ecological functioning of San Francisco Bay. The primary objectives are methodological rather than being focused on hypothesis testing. The authors plan to analyze archived benthic samples to convert species abundances into more ecologically useful currencies such as biomass and production.</p> <p>This is an unusual proposal (at least for my range of experience) in that no new samples will be taken and analyzed, and no hypothesis will be tested. This would usually result in a proposal being a non-starter, but in this case, I think the authors have made the case of the potential importance of archived samples for critical testing of theories of ecosystem function.</p>
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

#0205: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Technical Review #1

Comments	<p>The authors have made an excellent case for the need of appropriate benthic data. Invasions of exotic species have clearly changed ecological processes in SF Bay, and the effects are obvious in the plankton and in the benthos. Better use of existing benthic data, particularly from long term sites, is essential. The conceptual models are very good.</p> <p>I did have some questions about Table 1--first of all, units for abundance are not given. The numbers are way too low to be abundance per square meter, and values for polychaetes are extremely low.</p>
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The main approach proposed here includes determining wet weight of archived samples, correct for loss of tissue weight during storage through the use of published values (a wide range, depending on species, length of storage, preservative), then convert to dry tissue weight using other published conversions. Production will be estimated from published P/B values.</p> <p>I have a few comments on this approach. First, I recommend that the authors consider conducting a few experiments to determine tissue weight loss during storage of preserved animals--they would then have much more confidence in making a correction. Second, I think the biggest problem lays in the conversion of wet weight to dry tissue weight, due to the importance of mineralized hard</p>
-----------------	-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Technical Review #1

	<p>parts. This is especially important for bivalves--it is known that shell weight can vary independently from wet (or dry) tissue weight, and the relationship between shell weight and tissue weight changes seasonally and with body size. I strongly suggest that the authors do some destructive sampling of archived bivalves, separating shell from soft tissue. It is likely that wet tissue weight will convert nicely to dry weight and on to carbon or biomass. All bets are off if the shell is included. In my opinion, this is the weak link of the proposal, but it is easily remedied.</p> <p>The other question, noted below in the budget section, focuses on the analytical nature of biomass analysis. This needs clarification.</p> <p>The other major components of the approach focus on data management and analysis. This section seemed fine.</p>
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>The project is certainly feasible, as long as authors add direct measurement of tissue wet weight to their measurements. Without this, bivalve data will be problematic. For soft-bodied animals, the proposed approach should be fine.</p> <p>I suspect that P/B calculations will be little better than good guesses. Too many variables contribute.</p>
Rating	very good

Technical Review #1

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Monitoring is not really applicable to this proposal.
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Adding appropriate and usable data about the benthos to an existing data base is quite a valuable product, especially for shallow, estuarine habitats to be examined here. The "Benthos Bioguide" is a nice idea, and is likely to be used by a wider audience than the data base. Nevertheless, access to appropriate data in a data base would allow hypothesis testing.
Rating	very good

Additional Comments

Comments	I look forward to seeing the data be incorporated into hypothesis testing. I would not be surprised to learn that <i>Potamocorbula</i> reduces deposit feeder dominance, but actually increases deposit feeder abundance through increase organic carbon biodeposition. Other infaunal suspension feeders have been shown to increase deposit feeder abundance.
-----------------	-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

#0205: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Technical Review #1

Comments	The authors are qualified to conduct this research and to manage and analyze the data. It's more difficult to comment on infrastructure, but they appear to have developed a valuable collaboration.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The proposed project is labor intensive and and budget is consistent with that. The only question I have is for the cost of biomass measurement (Task 1). \$50,000 is budgeted for biomass analysis for 500 samples, but I cannot find in the proposal exactly what analysis will be conducted. The cost per sample is too high for CHN analysis, although such measurements would make sense here. Some clarification is needed here, but otherwise the budget appropriately matches the scope of the project.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	Overall I am favorably inclined toward this proposal because it addresses an important need, and may make archived samples much more useful in ecological testing. Therefore I rate the proposal highly even though it lacks a central hypothesis. Primarily because of concerns about methodology, but also due to questions about budget, and dismay over some sloppiness throughout the text (missing references, repeated lines, I have reduced my rating to very good.
Rating	very good

Technical Review #2

proposal title: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Francisco Estuary

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The title of this proposal is understated to the point of being a bit misleading. It offers much more than "trends in benthic biomass"; in fact, production, respiration, and assimilation efficiency all will be determined (with reference to literature values).</p> <p>Having made this (inconsequential) quibble, I found the text of the proposal clearly laid out its objectives and hypotheses to be tested with the data generated.</p> <p>The idea is timely--even overdue--it is very surprising that these biomass data have not been generated before in the decades of SFE and delta monitoring!</p>
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	<p>According to the proposal (p. 2), CDWR (2003) "strongly recommended incorporating biomass measurements into this (EMP) program". While I have</p>
----------	------------------------------------------------------------------------------------------------------------------------------------------------------

#0205: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Technical Review #2

	<p>not read the CDWR document, I agree. Biomass information is fundamental to any full appreciation of benthic processes.</p> <p>There are conceptual models (summarized in Figs. 4 and 6) that explain the context of the proposed work.</p> <p>The selection of a representative subset of the more than 10,000 benthic samples available is made clear and seems appropriate.</p>
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Assuming the fundamental issue of a consistent mass measurement can be resolved (see comment in "feasibility" section), the results anticipated by the proposers certainly will add important information to the SFE/delta database useful in making management decisions.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	1) On first reading, I had some considerable skepticism about the PIs' ability to weigh these samples accurately to the nearest 0.0001 g (p. 9). To that end, I performed a couple tests using plankton tow samples. In the first, I weighed shrimp larvae (n=3 specimens)
-----------------	----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Technical Review #2

and in the second, isopods (n=2); both species had been preserved in ethanol. In each case, I dried the specimens on Kimwipes until their transfer to a new section of the tissue no longer resulted in a wet spot (ca. 1 minute), consistent (I believe) with the procedure outlined on p. 9 of the proposal. After placing the shrimp on a weighing boat, their indicated mass dropped in 90 seconds from 0.0032 g to 0.0000 g as they fully desiccated in the enclosed weighing chamber. Results with the isopods were not as dramatic, but again, there was a decrease over 90 seconds, from 0.0094 to 0.0079 g. The balance is a Denver Instrument unit, XE series, Model 100A and was last serviced 6 months ago.

While I by no means suggest these simple measurements refute the recommendations of the Southern California Bight Monitoring Manual (p. 9), I do urge very serious appraisal of the proposed accuracy of 0.0001 g. Did Peterson (2002) use this level in her thesis? This proposal would be stronger were it to offer evidence that this level of accuracy can be consistently and meaningfully achieved.

Would an accuracy of 0.001 g (ten-fold less) impair the sensitivity of these analyses? I pose the question, but cannot answer it without knowing the biomass of the samples involved.

2) Another concern of mine has to do with the "population notes" to be made at the time of the weight estimate to "record observations of the number of cohorts and relative size range of animals" (p. 9). These aren't casual observations to my mind. How many size bins? How will small species be measured? Will an "eyeball approach" really work with spionid

Technical Review #2

	<p>polychaetes, for example, given that their size often is expressed as width of a specific setiger? These questions (and others like them) make the point that a rigorous approach to sizing these invertebrates would add a tremendous labor load to the project, one not anticipated in the budget. The question is, therefore, will the PIs' non-rigorous approach serve their purpose?</p> <p>3) How will the PIs handle the issue of deposit feeders, at least those full of sediment? All other things being equal, they will have a lower C:mass ratio than those species that are not deposit feeders.</p>
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	<p>The study itself is not a monitoring project, so the first of these questions does not apply. However, the entire point of the proposal is to increase the value of the existing abundance data by adding the corresponding biomass data to a representative subset of the samples. In turn, these combined metrics would serve to greatly increase interpretation of the SFE and delta monitoring data.</p>
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Technical Review #2

Comments	<p>This is a potential goldmine of products. The fundamental information, biomass, is itself inherently publishable, but the subsequent cascade of information (production, respiration, etc.) would make for many substantial and widely read publications in the scientific literature. Furthermore, these products would be useful to managers as well.</p> <p>With respect to contributions to "larger data management systems", the PIs have in fact devoted one of their tasks (#2) to the export of their data to the EMP Benthic database (p. 10).</p>
Rating	excellent

Additional Comments

Comments	<p>I suggest the PIs incorporate into their thinking the data and conceptual models available from Great Lakes colleagues, especially those relative to changes in nutrient and contaminant cycling as a result of zebra mussels' suspension feeding.</p> <p>A weak point in this proposal is its inclusion of sweeping statements (appropriately made in their conceptual model) that are not subsequently amplified, e.g., "Measurement of estimated biomass within populations of benthic organisms... will allow us to assess rates of biological activities, such as filter feeding, transfer of materials across the water-sediment boundary, and transfer of contaminant material from the benthos into other portions of the food web in the system" (p. 3). Yet the PIs do not indicate further how these rate assessments will be made, even in the case of suspension feeding, which is one of their Task 3 objectives (p. 11). Grazing rate of suspension feeders will be estimated "as described by Thompson (2004) and values will be analyzed as time series." Unfortunately, there are no details provided and Thompson's book chapter is listed as "in press".</p>
----------	-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Technical Review #2

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Key personnel who would be supported by this project have a long involvement with the monitoring programs of the delta and the estuary. They have the tools and the smarts to perform this project and see to its dissemination.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is reasonable for the work proposed.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	<p>This is a compelling proposal having a central idea that practically "sells" itself. The project personnel are experienced and largely in place. The budget is reasonable for the proposed tasks. I especially want to point out the PIs' appreciation of error analysis, e.g., p. 9 and p. 12. I applaud them for their thinking in this regard.</p> <p>I expressed above my concerns with the accuracy of weighing and sizing these invertebrates. I think these issues could be addressed quickly either with detailed reference to Peterson's (2002) methods or by empirical studies.</p>
Rating	

Technical Review #2

	very good
--	-----------

#0205: Long-Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Technical Review #3

proposal title: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Francisco Estuary

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The goals of the proposed research are very sound. The authors make a reasonable case that biomass measurements are needed, and in fact, biomass measures are common in most monitoring efforts. There are no hypotheses, and so it is not possible to rate them.</p> <p>(1) It is very important that the consistency and sample quality are good, i.e. that the samples are fully quantitative (e.g. boxcores and not grabs) and consistent over time. The sample methodology is not stated in the proposal so it is difficult to evaluate.</p> <p>(2) The P.I. implies that “ecosystem-based” management reflects carbon cycling and the interrelationship (exchanges) among various components that cycle carbon and nutrients. While this may be true to some degree, the P.I. chooses a very narrow definition. In the broad sense, ecosystem-based management includes examining species in relation to other aspects of the ecosystem which may include the distribution and abundance of individuals of the same species, of competitors, predators, etc... and is not solely defined as a carbon-based approach. The authors state that species and abundance patterns may differ from biomass patterns, but they should stress more forthrightly that both types of information are useful for ecosystem-based approaches.</p>
Rating	very good

#0205: Long–Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Technical Review #3

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>Conceptual models are provided and I have the following comments on them:</p> <p>Comment: I disagree with the statement that knowledge of biomass will allow calculation of rates of activity such as filter feeding and sediment seawater exchange. Filter feeding is far more a function of the number of individuals and their filtration rate, and this is the data that is required to do these calculations. For sediment-seawater exchange, one must look at activity rates and burrow characteristics, which may not be related to size at all but rather, behavioral characteristics and sediment type. I feel that biomass is best used in conjunction with estimating transfers of material because biomass is the embodiment of that material! The other calculations alluded to by the P.I. would require significantly more information than is currently contained in the proposal.</p> <p>I also disagree with the rationale provided for the relative abundances of deposit feeders vs. suspension feeders. It may have as much to do with predation of deposit feeding larvae by suspension feeders, or alternatively, trophic group amensalism, than the hydraulic residence times or organic matter residence times on the bottom. Although I disagree with several aspects of the conceptual models, I feel it is useful to know the relative biomass of suspension vs. deposit feeders and so I am supportive of the overall goal.</p> <p>Finally, many of the assumptions and assertions lack the appropriate citations from the primary literature and, unless instructed otherwise by the agency, the P.I. should make every effort to substantiate the scientific assertions and statements contained within</p>
----------	-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Technical Review #3

	the proposal body.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>Objective 1: The first objective is at odds with the overall goals stated in the introduction. The overall goals are to develop a protocol for determining biomass. However, the first objective seems to sidestep the "development of a protocol for estimating biomass". In this section it appears that the P.I. has already decided which method is to be used, and that biomass will not be "measured" as stated in the introductory section, but estimated. The P.I. needs to be more clear regarding the goals and approach. As the P.I. has already decided which method is best, the development aspect no longer applies, and one must hope that the P.I. has chosen well. I don't have enough experience in this aspect of biology to determine whether the chosen method is the best. If good estimates of biomass are obtained, the information will be useful to modelers and ultimately, decision makers, assuming the models are reasonable and have good predictive value.</p> <p>Objective 2: The approach for making the data available to the public and researchers is well-conceived.</p> <p>Objective 3: Estimation of production requires a sampling effort that allows one to track the growth of a community. For benthos this should</p>
----------	------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Technical Review #3

	be on the order of monthly, and it appears from the choice of samples that this frequency of sampling has occurred. As for the modeling effort, the models rely heavily on a number of simplifying assumptions and I don't know if these assumptions are reasonable because the model details are not available.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The data analysis section has no information on the statistical methods to be used to evaluate and compare the data. This is a significant omission. As such I cannot evaluate whether the methods are sound and will produce useful results. On the basis of the lack of information, I rate this section as "fair".
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre-post comparisons; treatment-control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	There is a compelling need to interpret monitoring data and estimation of biomass is an important addition to this effort.
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

#0205: Long-Term Trends in Benthic Macrofauna Biomass in the Upper San Franci...

Technical Review #3

Comments	The biomass estimation, if done properly, will be very useful as will the publications and outreach components.
Rating	very good

Additional Comments

Comments	Again, I think obtaining the biomass estimation is useful but I feel the authors have reached a bit too far in suggesting what they can do with it. Models are not fully discussed so it's impossible to evaluate the overall utility of the modelling effort that will use this data.
----------	----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Publications are not abundant among the individuals but it may be the case that they are not required to publish as part of their employment. The proposed staff seem competent.
Rating	good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems very high. However it's hard to tell how difficult it will be to process the samples because it's not known how abundant the individuals are in each sample. The value of the product should be compared to the funds already invested in the benthos monitoring effort - if it is small by comparison, it is probably well worth the investment. I have no idea what the other agency priorities are so I don't know how compelling this project is relative to others
----------	------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

Technical Review #3

	proposed.
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	It is entirely reasonable and appropriate to have the biomass data and on that basis I rate the proposal as good. It is not scientifically groundbreaking but it may have significant benefits for applied science.
Rating	good